

March 27, 1952

Dr. Rollin Hotchkiss
Rockefeller Institute for Medical Research
New York 21, N.Y.

Dear Rollin:

I haven't given very much thought to either the Boston or Baltimore meetings, as I don't expect to be attending either. I talked with Ed Tatum a couple of weeks ago about the former. As you say, the program looks very crowded. Zinder will be giving a short paper there in the general session anyhow; if there is time and the occasion really demands it, perhaps he could add to the discussion from the floor. But this should not influence your plans. As to Baltimore, probably neither of us will be there after all. I don't see how we can really "discuss" your paper under the circumstances. If you thought it desirable, why don't you yourself quote our experiments (and if you can find it, our point of view) at the meeting? We could have a private discussion by mail. If this seems to have dialectic possibilities, we could arrange a dialogue for the publication (as Mac seems to want). Whether or not we do this, I would hope there would be an occasion to discuss: 1) that the "transformations" are genetic transductions (per Webster's dictionary) and not induced mutations as often understood, and 2) the extent to which a rigorous view of the experiments leaves room for an organized element in TP, on the one hand, or a protected active component (DNA?) in FA on the other. The genetic complexity of III-capsular TP, and the newly revealed scope of both TP and FA are, I think, pertinent. Or are we to think of crossing-over in "molecules"? I think you are quite right that we are looking mostly at the vehicle in FA (and perhaps TP also?), and DNA or DNA-protein would be as good a bet as any for the active component. There may be a bit of a technical problem in the adsorption and penetration of the stripped FA, even if the stripping is possible. Should we try serum? No cofactors are needed for FA now.

If you do cite Salmonella transduction, may I ask that you emphasize that E. coli sex is altogether another story. As often as I do this, it still comes back garbled.

Zinder should win his Ph.D. this spring, and has no commitments here. I would hope that he will continue Salmonella studies wherever he goes. On the other hand, I've been deeply interested in Salmonella for some six years, and expect to continue our program, probably along "biological" lines. I appreciate your courtesy in consulting me about your plans, but see no occasion for any conflict that we can't avert by open discussion. Zinder will have to make up his own mind if he is formally approached by both Schneider and yourself. I think that it's really too bad if a de facto arrangement couldn't be worked out that would let him share the two contacts. Alternatively, would there be any point or possibility of a scheme to spend one year with you, and then go over to Sch? I haven't discussed any of this with Zinder, but at this distance it sounds worthwhile. He would like to learn some bio-physics and -chemistry before deciding or settling his plans, but I think the advantages might be reciprocal.

Yours sincerely,

Joshua Lederberg